Empowerment for Whom? The Impact of Community Renewal Tax Incentives on Employment, Establishment Openings, and Closures in Urban Neighborhoods.

by

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Abstract

Some postulate that spatially targeted grants and tax cuts stimulate jobs, establishment openings, and reduce closures in distressed urban neighborhoods. Scholarly literature is mixed and mostly argues that at best these programs have no impact and at worst raise land rents spurring gentrification. The United States designated three rounds of Renewal Communities, Empowerment Zones or Enterprise Communities (RC/EZ/EC) to receive wage credits or grants. While other researchers have estimated the impact of Round I EZ/ECs, this article will contribute by estimating the impact of more recent rounds in Tennessee and California on job and businesses using propensity score matching. Data are presented by RC/EZ for retail, very small, and minority establishments. Jobs increased in target areas compared to control areas during the wage credit period, in particular for businesses with five or fewer employees located. In general, establishment closures and openings both fell. Policy recommendations end the article.

Keywords: Community Economic Development, Tax Incentives, Urban Policy, empowerment zones, job creation

Policy makers struggle with solving the problems of concentrated poverty and unemployment in inner cities. In the United States, the urban anti-poverty continuum swings between programs that focus on investments in people in poverty (people-based programs) and those that target investments in neighborhoods (place-based programs) with high poverty (Ladd, 1994). The former include traditional welfare cash transfers and public education while the latter often take the form of direct investments in public infrastructure and public housing (Spencer, 2004).

From 1994 - 2011, the United States Department of Housing and Urban Development (US HUD) administered three rounds of place-based initiatives called Renewal Communities, Empowerment Zones or Enterprise Communities (RC/EZ/EC)(US HUD, 2012). Benefits included different packages of tax incentives or grants that varied in each round. While other researchers have estimated the impact of Round I EZ/ECs, this article will contribute by estimating the impact of Round II EZs and Round III EZs and RCs in Tennessee and California. The outcomes analyzed in this paper are jobs, new establishments, and establishment closures and they are estimated using an adaptive algorithm used for observational studies in biostatistics.

The first part of this paper will review the evolution of the policy as it moved from the United Kingdom, to various sub national governments in the United States to US HUD. The second part of the paper will briefly review methods used to research place-based policies generally and the US HUD programs specifically. The paper will then present the method and results for the sample analyzed and conclude with policy recommendations.

The Renewal Communities (RC)/Empowerment Zones (EZ)/Enterprise Communities (EC) Program

There has been a half-century-long history of place-based urban revitalization initiatives in the U.S., including: (1) urban renewal in the 1950s (Halpern, 1995); (2) Model Cities in the 1960s (O'Connor, 1999); (3) Urban Development Action Grants in the 1970s (Rich, 1989); and (4) state enterprise zones in the late 1980s (Lavin & Whysall, 2004; Engberg & Greenbaum, 1999) and Comprehensive Community Initiatives in the late 1990s (Kubisch, 2010). The most recent federal initiative in this series was the Empowerment Zone / Enterprise Community (EZ/EC), which is the subject of this paper. In 1994, the Federal government likewise returned to an interest in place when US HUD designated eight Empowerment Zones and 65 Enterprise Communities in order to build capacity and increase economic opportunity in high poverty neighborhoods (Lavin & Whysall, 2004). See Table 1 for a summary of benefits, which included social services grants, loan guarantees, and tax incentives for businesses (US HUD, 2012). In particular, the EZ wage credit allowed a business to take 20% of qualified wages up to $3000 off their taxes for each employee they hire who lives and works in the designated area. This emphasis on place and people was a clear attempt to encourage businesses to hire from the EZ. In 1997, US HUD designated 15 Round II EZs. Later in 2001, US HUD designated eight Round III EZs and 40 Renewal Communities that only received tax incentives (US HUD, 2012). The RC wage credit was for 15% of qualified wages capped at $1500 per employee. Congress also extended the Round I EZs to match the expiration date of Round III.

Local governments partnered with different organizations to administer or promote EZ and RC benefits For example, many EZs partnered with the Small Business Administration One-Stop Capital Shop in order to ensure that entrepreneurs and employees in the EZ would have access to business assistance services and information about obtaining financing (US HUD, 2005). Annual reports from the RCs and EZs describe outreach to minority and immigrant

businesses (US HUD, 2008). A U.S. Government Accountability Office (US GAO) (2004) report noted an increasing utilization of tax incentives between 1995 and 2001 in contrast to initial findings in US GAO (1998). By 2008, businesses claimed $3.2 billion in RC and EZ wage credits, benefited from $643 million in tax-exempt bonds and were able to accelerate the depreciation of $1.7 billion of new commercial real estate (US GAO, 2010). Hanson (2009) estimated that employers took wage credits for 24.2% of employees in the EZ.

<Insert Table 1 about here>

Eligibility and selection criteria for this program were complex. To apply for the EZ/EC program, local governments prepared strategic plans for sustainable community development with substantial participation from neighborhood residents and other stakeholders (24 CFR Part 597). The most important eligibility criteria was that RC/EZ/EC areas had to be selected from 1990 census tracts with at least 20% poverty and high unemployment, with some exceptions. In the RC program, unemployment needed to be at least 9%. For Round II and III EZs, Congress relaxed the eligibility criteria, such that designed areas were not from the poorest census tracts (Greenbaum & Bondonio, 2004). After the EZ/EC applicant met objective eligibility thresholds, US HUD had a panel of reviewers subjectively score the quality of each strategic plan. In contrast, RC selection was objective and based solely on ranking applicants' poverty and unemployment with a bonus point for having been an EZ/EC (See 24 CFR Part 597-598 and US GAO (2004) for more details on selection criteria).

Review of Empirical Research on EZs

Both equity and efficiency concern are raised in the literature about place-based policies was that they are inefficient. The subsidies would became capitalized in land rents and pass on

the costs to inner city residents, who would have been better served with investments in schools, income transfers or enforcement of anti-discrimination policies (e.g. Quigley, 1994; Lavin & Whysall, 2004; Levine, 1999). However, others reject this zero sum game argument and point out that a region could be better off if resources are expended to create jobs in a high unemployment area (Bartik, 1991). Evaluations have estimated the impact of state and federal EZ programs on inventory, real estate capitalization, wages, vacancy, poverty, and unemployment. In the 1980s, researchers evaluated state enterprise zones (SEZs) using descriptive techniques that included case studies, shift-share analysis, or micro-simulations, subjecting them to criticism of omitted controls, selection bias and non-generalizability (Wilder & Rubin 1996; Ladd 1994; Boarnet, 2001). Identification of a causal policy impact for these programs was confounded because the selection of winners was done based on need and not randomized. Evaluators of SEZs subsequently used three identification strategies that later were applied to Federal EZ/ECs (FEZ).

Instrumental Variables and other Panel Data Methods

Early evaluations compared jurisdictions with SEZs to others without them using panel data regression controls for fixed effects and a random growth curve to allow each observation to have its own rate of change (e.g. Papke 1994). Some of the earlier studies did not capture the exact geography of the SEZ due to data limitations and justified this choice arguing that the policy should lead to a net increase in employment in the jurisdiction (e.g. Bondonio & Engberg, 2000). Some SEZs studies with fixed effect controls find no effect on employment (e.g. Boarnet, 2001, Elvery 2009). For the Federal EZ program, Hanson (2011) used political and economic

instrumental variables to control for selection bias and found no difference in employment outcomes but did find an increase in property values.

Regression Discontinuity

The second identification strategy used a regression discontinuity design to identify the local average treatment effect along the border of the EZ. For example, Hebert et al. (2001) used data from Dun and Bradstreet and found increased job growth in four of the six FEZs and an increase in the number of minority owned businesses using a pre-post design with a comparison group comprised of adjacent high poverty tracts in the same city. In a study of both state and federal EZs, Ham, Imrohoroglu & Swenson (2009) argued that previous matched-pair analysis along the border of the EZ did not adequately distinguish between policy effects and locational advantage, so they augmented the RD with a random growth curve model to account for baseline conditions and found a positive effect on employment. One recent evaluation of California's FEZ and SEZ program used the National Establishment Time Series (NETS) database, an annualized extract of Dun and Bradstreet data beginning from 1990 that links a January snapshot of each business respondent longitudinally (Walls, 2008). These data are self-reported from Dun and Bradstreet's survey of business owners. Kolko et al. (2009) found no impact on jobs using a regression discontinuity design with adjustment except for those areas that also had been designated FEZs. They tentatively concluded that the added value of grants and staffed professionals may have made the difference in program implementation.

Propensity-score Matching

The third identification strategy used was propensity-score matching to identify control areas (Dehejia & Wahba 2002; Rosenbaum & Rubin 1983). This allowed estimation of a

parameter called the average treatment effect on the treated. For example, O'Keefe (2004) conditioned on variables such as race, poverty, unemployment, or vacancy rates to find matches within the same jurisdiction in order to prevent confounding from unobserved differences across jurisdictions. Arguably, cities had many unobserved differences. Oakley & Tsao (2006) compared Round I EZ tracts in Baltimore, Chicago, Detroit and New York to other distressed tracts within the same city using Census data from the Neighborhood Change Data Base. They concluded there was no impact on logged average household income, unemployment, and poverty for each EZ separately and also pooled the estimates. However, in some instances, the EZ included most of the distressed area in the jurisdiction. As an alternative approach, Busso & Kline (2006) used Census microdata to identify comparison areas from eligible losing applicants for an FEZ designation in other jurisdictions and found an overall increase in employment and reduced poverty for Round I EZs. Busso & Kline (2006) rejected within city comparisons and RD because of spillover effects and the selection bias of choosing to apply to the EZ and being selected by US HUD. The US GAO (2006) used data from Caliper and found improvements in the number of businesses in FEZs in Camden, Cleveland, and New York compared to matched areas before treatment. US GAO (2006) also found an increase in job growth in Baltimore, Camden, Detroit, Upper Manhattan, and Philadelphia, but decreased jobs in Atlanta, Bronx, Chicago, Cleveland, Los Angeles, and Philadelphia. US GAO (2006) was not confident in attributing a causal relationship because their control areas did not have covariate balance, common support, or a reasonable qualitative justification based on their conversations with zone administrators.

In contrast, Rich and Stoker (2010) argued that a heterogeneous treatment effect assumption is plausible because cities operate under different institutional contexts. They

identified matches within the same city, but estimated the average treatment effect on the treated (ATT) for each Round I EZ as a separate case using robust bootstrapped standard errors. They argued that most of the FEZs had positive outcomes for poverty, unemployment, housing starts and business and that having large effect sizes was more important than statistical significant because of the small sample size and policy makers' need for practical results.

In short, while program expenditures have been large, the results of these programs have been mixed. This study adds to the literature by being the first to analyze the impact of Round II EZs and Round III EZs and RCs. I am analyzing the NETS data with a state of the art propensity score matching tool called genetic matching (GenMatch) and interrupted time series analysis to improve estimates of the policy impacts. Finally, this study is one of the first to examine the impact of the incentives on the subpopulations of retail firms, very small firms, and minority businesses.

Specifically, this paper tests three hypotheses:

Hypothesis One: The RC/EZ/EC areas have a one time increase in jobs and an increased growth rate in the post intervention period compared to the pre-intervention period and the control area (JOBS).

Hypothesis Two: The RC/EZ/EC areas have a one time increase in the number of new establishments and increased establishment opening rate in the post intervention period compared to the pre-intervention period and the control area (OPENINGS).

Hypothesis Three: The RC/EZ/EC areas have a one time decrease in the number of closed establishments and decreased establishment closure rate *compared to the pre-intervention period and the control area. (CLOSURES).*

Data and Methods

Study Sample and Data Sources

Since 1995, US HUD has designated 117 RC/EZ/ECs. Given resource constraints, I was limited to two case study states, which I selected because they each had cities with Round II EZ

and RC designations. The sample came from businesses located in the RC/EZ/ECs in the following areas: Memphis, TN (RC); Knoxville, TN (Round II EZ); Chattanooga, TN (RC); Los Angeles, CA (RC); San Diego, CA (RC); San Francisco, CA (RC); Fresno, CA (Round III); and Santa Ana, CA (Round II). See Table 2 for descriptive statistics of the study sample.

<Insert Table 2 about here>

Dependent Variables

The three outcomes analyzed in this study are jobs, new establishment openings, and establishment failures. The unit of observation is the census tract. The dependent variables are taken from the NETS to operationalize these outcomes. NETS supplies a latitude and longitude for firms, which I joined in ArcGIS to 1990 census tracts. The counts for each of the dependent variables were summed for each 1990 census tract.

First, jobs are measure by the number of employees reported by the establishment at the location of business (Emp1990–Emp2007). Second, NETS has a field for new establishments that gives the first year active in the NETS from 1989–2006 (FirstYear). Third, establishment closures are computed in the NETS by taking the last year reporting as the last year open from 1990–2006 (LastYear). The NETS distinguishes business moves from business starts and closures.

Firms that were not eligible for tax incentives (government, non-profit, country clubs, hot tub facilities, suntan parlors, gambling, massage parlors, and liquor stores) were removed because they could not have any direct effects from the wage credit (IRS, 2004; US HUD, 2003). Unlike Busso, Gregory & Kline (2011), I included establishments with fewer than five employees because during the study period consultants aggressively marketed the wage credits

(ADP 2010; FIRSTAdvantage 2010). Because about 50% of employees work for firms with five or fewer employees, the data were subset accordingly, to see if these smaller establishments were sensitive to the effects of the program. Retail firms are displayed separately because they are labor intensive and therefore would be more likely to take a wage credit. In order to see if these incentives are reaching minority businesses, I filtered the data set using the NETS' self-reported minority owned variable.

Treatment Variable

There are two levels of treatment in the study sample[1]. The treatment variable equals one if a census tract was designated an RC or EZ in a given year (WAGE CREDIT) and zero otherwise. For pooled estimates, an additional variable distinguished the Round II EZs (Santa Ana, CA, and Knoxville, TN in this sample) that each received $26 million in economic development grants (EZ GRANT)[2]. Because each RC or EZ customizes the treatment performed in each community, separate analyses are presented for each.

Matching Variables

The variables listed in this section are used to match a set of control census tracts for each treatment census tract and are not variables of interest. The first set of matching variables chosen mimics official selection criteria used by US HUD and include poverty, unemployment, population, logged area, and location in a central business district (24 CFR Part 597). The logged population density and area were included to create a variable with a near normal distribution to

[1] The Los Angeles EZ was excluded because it was award by Congress at the request of the HUD secretary. In other words, it did not have the same propensity score function as the others.

[2] While Round I EZ/ECs followed census tract boundaries, Congress allowed up to three non-contiguous industrial or commercial parcels to be designated as developable sites in Round II & III EZ (24 CFR 598). As a simplifying assumption, the entire tract is included as a treatment tract if it encompasses a developable site. This assumption biased estimates towards zero by including non-treated businesses in the sample.

facilitate the matching algorithm. The change in Census unemployment and poverty from 1990 to 2000 were included because local government planners may have included areas based on known local trends that would not have been obvious in the 1990 data. To simulate the requirement that tracts be adjacent, I used the Moran's I local spatial autocorrelation statistic and the corresponding cluster result calculated in GeoDa (Anselin, Syabri & Kho, 2006) from 1990 poverty because each tract in the EZ or RC must be contiguous and have at least 20% poverty.

The next set of variables is chosen to reduce bias in the study design. I include the percent minority (i.e. African American, Hispanic/Latino, Asian, and Native American) in 1990, and percent of the population that is foreign born. Another matching variable included having a U.S. representative in the majority party, to control for political representation in the event that Congress influenced selection (Wallace, 2003; 2004). I included a dummy variable for being a Round I Enterprise Community to ensure that treatment and control tracts are not confounded by lingering treatment effects of the Round I EC program[3]. Finally, the 1990 value of each pre-treatment outcome variable was included to ensure a conservative test of the treatment effect.

Data Analysis Methods

In this study, the unit of observation was the census tract. As with previous work (Busso, Gregory & Kline, 2011; Rich and Stoker, 2010) this study used propensity score matching to make claims about impact. Matching allowed ATT to be estimated provided that the control group that satisfies the selection on observables assumption, which required that the treatment and control groups had no statistically significant differences on all variables that could have influenced selection. Covariates were considered balanced if $p > .10$ on all key variables using t-

[3] Busso, Gregory & Kline (2011) restricted control tracts from the Round I EC winners and losing but eligible applicants. In California and Tennessee, there are not enough Round I ECs tracts to serve as controls.

tests and Kolmogorov–Smirnov tests. A second condition required for estimating ATT is called common support (Sekhon, 2009), or as Rosenbaum and Rubin (1983) say, strong ignorability. In other words, the probability of treatment assignment conditional on potential confounding variables was unbiased for the each group. This happens if the distributions of all variables were similar for the treatment and control groups and were assessed using kernel density plots.

The RC selection criteria were objective so satisfying the selection on observables assumption was trivial by conditioning on poverty, unemployment, population, and area. However, the US HUD selected EZ applications subjectively based on the quality of their strategic plan. Furthermore, this policy was particularly subject to stable unit treatment value assumption violations in that spill-over effects may have occurred when grant expenditures and tax incentives benefited those outside the area.

Because the RCs had different selection criteria (24 CFR Part 599) than Round II and Round III EZs (24 CFR Part 598), I estimated separate propensity scores. GenMatch software selected matches to maximize covariate balance between treatment and controls (Diamond, & Sekhon 2005; Sekhon, 2009). This design used one-to-many matching with replacement and weighted the controls proportionately to improve the estimate and obtain correct inference. In this study, the control tracts were drawn from all counties designated urban by the U.S. Department of Agriculture rurality continuum for Tennessee and California. Each RC/EZ/EC tract was matched to a set of tracts in the state in which it was located. This was done to prevent confounding with unobserved state level variables.

While propensity score matching may be used to identify a control group in an observational study, a simple difference in means may only be used in a pre-post design

(Hartman, 2009). Because the NETS data in this study contained 17 panels, I combined matching with the interrupted time series (ITS) (Morgan and Winship, 2007). In particular, to identify the impact of an intervention by controlling for past levels and trends, I used the adjusted interrupted time series (AITS) model from Galster et. al. (2004):

$$link(DV_t) = c + d_1(DIMPwc_t) + d_2(DIMPgrant_t) + e_1(DPOSTIMPwc_t) + e_2(DPOSTIMPgrant_t) + f(TRIMP_t) + g_1(TRPOSTIMPwc_t) + g_2(TRPOSTIMPgrant_t) + e_2(DPOSTIMPgrant_t) + f(TRIMP_t) + g_1(TRPOSTIMPwc_t) + g_2(TRPOSTIMPgrant_t)$$

The three dependent variables, DV_t, were the number of jobs, new establishments, or establishment closures as reported to Dun and Bradstreet among businesses located in a given census tract. The independent variables were the treatment period, area, and trend variables. *DIMP* was a time invariant dummy for the EZ or RC area (1 = yes, 0 = no). There were two levels of treatment in this sample (WC for wage credit and Grant). Accordingly, $DIMP_{grant}$ identified a census tract in an EZ that received a grant (Knoxville, TN and Santa Ana, CA), where $DIMP_{wc}$ identified all EZs or RCs because they all received a wage credit, only at different times. $DPOSTIMP_t$ was a dummy for an EZ or RC post impact date, and $TRIMP_t$ was a vector of cardinal numbers, starting at one for the first time period (1990 = 1) and increasing by one for each time period (2007 = 18 for jobs; 2006 = 17 for establishment openings and closings). Likewise, $TRPOSTIMP_t$ was a similar vector of cardinal numbers, but starting at the year the grant or wage credits went into effect. $TRALL_t$ numbered the trend in the dependent variable for tracts both inside and outside the EZ or RC while $TRPOSTALL_t$ numbered the trend in all tracts (only post award) of wage credits or grants, respectively. With regard to interpretation, the key variables of interest were *e*, the one time change in the level of the dependent variable, and *g*, the

change in the trend over time of dependent variable. Estimation was performed in Stata 10.3 using negative binomial regression (Hilbe, 2011; Hubbard et al., 2010)[4].

Results

See Figures 1 and 2 for means and balance statistics of the treatment and control groups. For the EZs, matching balance was above .10 on both t-tests and KS tests for all variables except the percent of residents who are foreign-born (t-test $p > 0.05$). For the RCs, matching was well balanced on both moments for key selection variables of 1990 poverty, unemployment, lagged poverty, and poverty clustering. The RC balance was also well balanced for t-tests between the two groups for lagged unemployment, percent minority, and 1990 jobs. The logged population density, logged area, and percent foreign born were not well balanced. Although the RC control groups is not as well balanced as the EZs, the AITS should provide a conservative estimate of impact by adjusting for past level and trends in the dependent variables.

Jobs

See Table 3 for a summary of pooled impacts of the wage credits and EZ grants impact on jobs, new firms, and establishment closures. For hypothesis 1 (JOBS), overall there was a 1.024 factor increase in jobs per census tract during the wage credit period. This appears particularly prominent in business with five employees or fewer, which saw a 1.212 percent increase in jobs per tract. On the other hand, the EZ Grant Period saw no significant change in jobs for all eligible business but did see a significant reduction in retail jobs by a factor of 0.949

[4] The AITS regressions are estimated using time series cross sectional negative binomial population averaged models because the dependent variables are count data, and have a lower limit of zero. These data are overdispersed (i.e. the mean is not equal to the variance) and this precudes use of Poisson regression. Estimates are from Stata 10.3, using xtnbreg population averaged model using generalized estimating equations with semirobust standard errors clustering assuming an autoregressive correlation structure.

and for firms with five employees or fewer by a factor of 0.938. The only significant change for minority owned businesses is the slowing down of the trend in job growth by a factor of 0.847 during the wage credit period.

Since each RC and EZ are customized by each local government, it is appropriate to analyze each separately. See Table 4 for the estimates of the three EZs in the sample and Table 5 for the RCs in the sample. For all eligible business, jobs increased in the wage credit period in all cities but Fresno and San Francisco. Retail firms saw job increases in each RC and EZ during the wage credit period. All but San Francisco saw a jobs increase for firms with five employees or fewer. All cities but Los Angeles saw a jobs increase in minority businesses. While minority businesses, experienced a decreased in the growth rate for jobs during the wage credit period in California, this slowing down does not appear to lower the overall level of jobs.

Openings

For Hypothesis 2 (OPENINGS) however, there was a reduction in establishment openings (See Table 3) during the Wage Credit period the number of new establishment openings fell by a factor of 0.580 for retail businesses only. During the EZ grant period, the number of new establishment openings fell for all eligible businesses by a factor of 0.660, retail business by 0.764, and firms with five employees or fewer by 0.603. During the wage credit period, annual trend of all eligible new firms decreased by a factor of 0.961 and by 0.952 for firms with five employees or fewer, but the annual trend in new retail firms increased by a factor of 1.134. During the EZ grant period, the annual trend of new establishment creation increased for all eligible new establishments by a factor of 1.045 and in firms with five employees or fewer by 1.065.

Examining each designation individually, the impact on the number of new establishment openings was mixed. During the wage credit period, the trend in annual jobs for all eligible business increased in Chattanooga and Memphis, but decreased in San Diego and San Francisco. Fresno, Knoxville, Santa Ana, and San Diego each had significant reductions in the annual trend of establishment creation. During the EZ grant period, the two cities' firms saw a decrease in new firms and establishment closures. The annual trend in new establishment creation increased in Knoxville and Santa Ana during the grant period. Also, Santa Ana saw a reduction in the annual trend of new minority businesses openings by 0.202 during the grant period.

Closures

For hypothesis 3 (CLOSURES) during the wage credit period, establishment closures for all eligible businesses fell by a factor of 0.926 with an accompanying reduction in the annual trend in establishment closures for firms with five employees or fewer by a factor of 0.966. Likewise, establishment closures for all eligible businesses during the EZ grant period fell by a factor of 0.836 while firms with five or fewer employees fell by a factor of 0.843. The only significant change in the annual trend of business closures was in Memphis. While Knoxville saw a 1896.3 factor increase in the number of establishment closures, this large effect size is a function of the short time window and small number of minority business in each census tract.

<Insert Figure 2 & 3 about here>

<Insert Table 3 about here>

Discussion and Conclusion

This study makes the following contributions to the literature on EZ-like place-based initiatives. First, other scholars have been analyzing the Round I EZs, while this study analyzed

Rounds II and III and the RCs. With the exception of Hebert et al. (2000) and US GAO (2006), most studies rely solely on US Census data to estimate impacts. This study uses panel data from the NETS in order to see change in level and trends over time. However, like previous studies on EZs, results are mixed in the sense that different participants and sub-classifications of potential businesses respond to the policy treatment in different ways.

Although covering different places and time periods, this study results are consistent with Ham et al. (2009), Busso & Kline (2006), and Busso, Gregory & Kline (2011) in that positive impacts on employment are detected. Interestingly, the key argument that Rich and Stoker (2011) make with respect to effect sizes being more important than statistical significance is moot here. Having additional panels produced more statistical power to have both large effect sizes and statistical significance in this study. This study also provides some data regarding how these incentives are utilized on the ground. Busso, Gregory & Kline (2011) argue that the mechanism came from capital investment through direct grants to stimulate an observable increase in jobs, but wage credits provide no observable effect, because they are too small or not utilized. The results from this study challenge this assumption in that certain kinds of businesses in some cities without grants also saw an impact on employment.

There are some possible post hoc explanations for differences in previous studies. Obviously, this study covers a different time period and different cities. It is plausible that the observed increase in jobs in very small firms is offset by job losses in larger firms which explains why previous studies to not observe an impact. It is possible that the increased outreach by the public and private sector convinced small firms to take the wage credit and in turn gave some comfort to hire one more employee, while large firms relied on standard procedures and responded rationally to the concurrent economic downturn to shed employees.

Because the tables report relative risks, a few back-of-the-envelope calculations illustrate the actual numbers of the impacts observed in this research design. For establishment closures in the wage credit area during the pre-intervention period there were about 1500 per year such that over six years we would expect 9000 lost establishments. During the intervention period there was a statistically significant reduction of establishment closures by a factor of 0.926 times 9000 expected firm closures yielding 666 avoided closures. In the grant areas, about 500 firms/year closed in the pre-intervention period. During the intervention period, there was a statistically significant reduction of establishment closures by a factor of 0.836 times 1672 expected firm closures yielding 328 avoided closures. A plausible post hoc explanation for the concurrent reduction in new establishments may result from having fewer closures and thus fewer available real estate vacancies for new establishments to use.

For job creation, in this sample 184 tracts received tax incentives only, including the 40 with the grants. If I multiply estimated effect size of 1.024 times an average of 2750 jobs/tract in wage credit areas, I see 2816 jobs/tract in the post intervention period for a total of 66 new jobs/tract times 184 tracts for an increase of 12,144 jobs. For the grant period the same calculation yields a small loss of 10 jobs/tract times 40 tracts for a loss of 400 jobs, but it is not statistically significant. The net increase in jobs would be 11,744.

Using wage-credit utilization rates reported in US GAO (2004), I project that zone businesses saved $990 million from 2001 through 2010. If I assume that the states benefit in proportion to their population, then 12% of that savings would go to CA for $118 million. Tennessee would capture 2% of that savings, or $20 million for a combined total savings of $139 million. If the only benefit one cared about was the direct job creation ratio, then one could think of the program as having a cost of about 11,744 divided by $139 million for $12,000 per job

created. If one consider the effect to be additive, then one may add the $52 million spent on the Round II EZ grants in these two states for a total of $195 million, bringing the cost-per-job total up to about $17,000. This would be well below the standard of $35,000 per job set by US HUD's Community Development Block Grant program (24 CFR 570.209(b)(1)(i)).

Limitations

This study suffers from several limitations common to any study of place-based initiatives and EZs & RCs in particular. First, the program is not randomly assigned, so at best we can estimate ATT. While the selection on observables assumption holds for the RCs in that all selection criteria are known and modeled, the assumption could be compromised in the EZ program because the subjective scores for the strategic plan are not released. Furthermore, comparable matches were difficult to find for the RC program because the regulations required selecting the most distressed submissions. That being said, in this study the internal validity of the design is biased towards false negatives.

Because the NETS is based on self-report data with unknown non-response bias, one needs to assume that measurement error on the jobs data are mean zero. An ideal study would have establishment-level data on tax utilization so that an Intent-to-Treat estimate could be distinguished from the effect of the treated on tax incentive users. These data are not available. I cannot claim any external validity to the full universe of Round II EZs and RCs, since I only investigate two states. As noted earlier, a larger sample would give the study more power to detect neighborhood-wide impacts of tax incentives. As in any research on neighborhoods, there are clear SUTVA violations (i.e. spillover effects), which this study attempted to control by matching on the spatial autocorrelation of the pretreatment outcome variable. Notwithstanding

the limitations, this is the first impact study on latter rounds of the federal EZ & RC program and first to exploit annual panel data rather than pre-post designs.

Implications for Policy

These data show that for very small businesses, a place-based tax incentive program can have a small, positive effect. If larger establishments take wage credits, they do not appear to have an impact on jobs for those establishments. Therefore, future federal area-based tax-incentive strategies should conduct outreach to very small establishments, especially those with limited English-speaking proficiency, to make sure that they get the benefit of the incentive.

The RCs expired in 2009 and the EZs were given a two-year extension to the end of 2011. The Obama administration is currently experimenting with three placed-based initiatives at US HUD (2010). The first is the Sustainable Communities initiative, a regional planning grant. The second is the Neighborhood Choice initiative, to fund public housing renovation. For the third, the Obama White House (2011) has asked Congress to replace EZs with 20 new Growth Zones (GZs). The GZ will be one contiguous area but otherwise following the procedures of the EZ (U.S. Department of the Treasury, 2011). However, the proposed GZ will only last four years. Given the results from this study, four years may be too short to see an impact. The GZ will include an accelerated depreciation schedule that allows businesses to take 100% of the adjusted basis in the first year of service. The GZ wage credit will credit employers 20% of the first $15,000 of wages of work performed inside the GZ by residents of the GZ and 10% of wages for those who live inside the EZ but work outside.

Finally, the new proposal is blind to race, ethnicity and nativity. The GZ could encourage partnerships with federal programs such as One-Stop Capital Shops, asset building, refugee

microcredit assistance and minority contracting programs. Furthermore, I recommend targeting areas that have a high proportion of adults with limited English proficiency in order to assist communities with a documented need for multilingual entrepreneurship assistance. Even with these proposed modifications, however, one should not forget that place-based initiatives relying on tax incentives, to the best of our knowledge, have a modest effect on employment, and are therefore no replacement for investments in education, health, and infrastructure. That is why it is important that after eligible applicants are identified, the final selection should be made randomly so that the impact can be measured accurately. The new law also needs to require proper data collection from the IRS to identify businesses who claim incentives at the GZ level so that compliers can be distinguished from non-compliers in an intent to treat framework.

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Table 1: Summary of Urban RC/EZ/EC Benefits

Type of Designation	Grants & Loans	Tax Incentives
Round I EC	$3 million Social Services Block Grants each.	Work Opportunity Tax Credit.
Round I EZ	$100 million in Social Service Block Grants each.	EZ Wage Credit, Capital Gains, Increased Section 179 Deduction, EZ Tax Exempt Bonds.
Oakland, CA EC	Same as EC, but additional Section 108 loan authority.	Work Opportunity Tax Credit.
Los Angeles EZ	No grants, additional Section 108 loan authority.	Same as EZ, but began in 2000.
Round II EZ	Economic development grants of about $15 million each.	Same as EZ, but began in 2002.
Round III EZ	None.	Same as EZ, but began in 2002.
Renewal Community	None.	RC Wage Credit, Capital Gains, Increased Section 179 Deduction, Commercial Revitalization Deductions began in 2002.

Figure 1: Empowerment Zone Balance Statistics

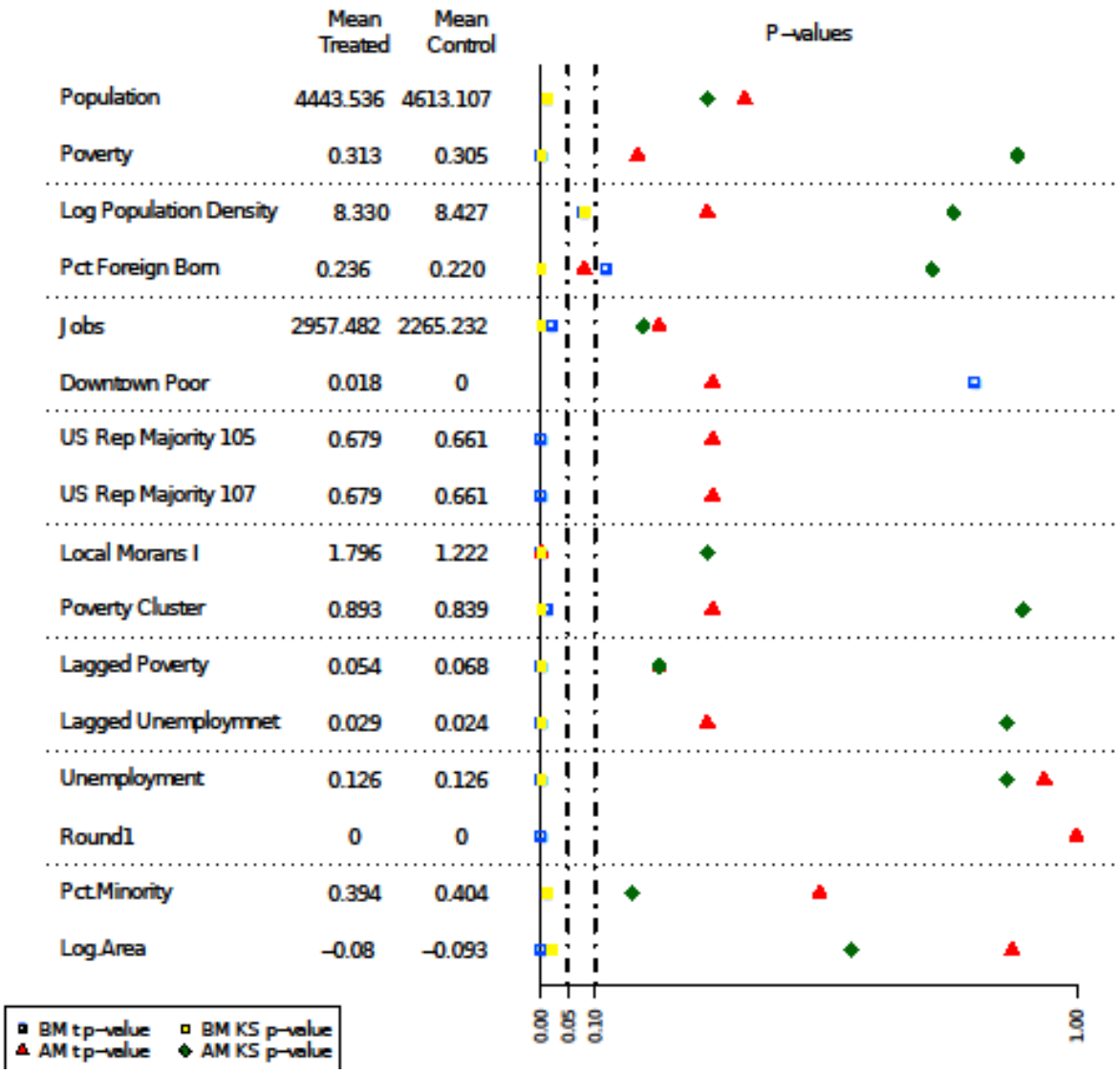

Figure 2: Renewal Community Balance Statistics

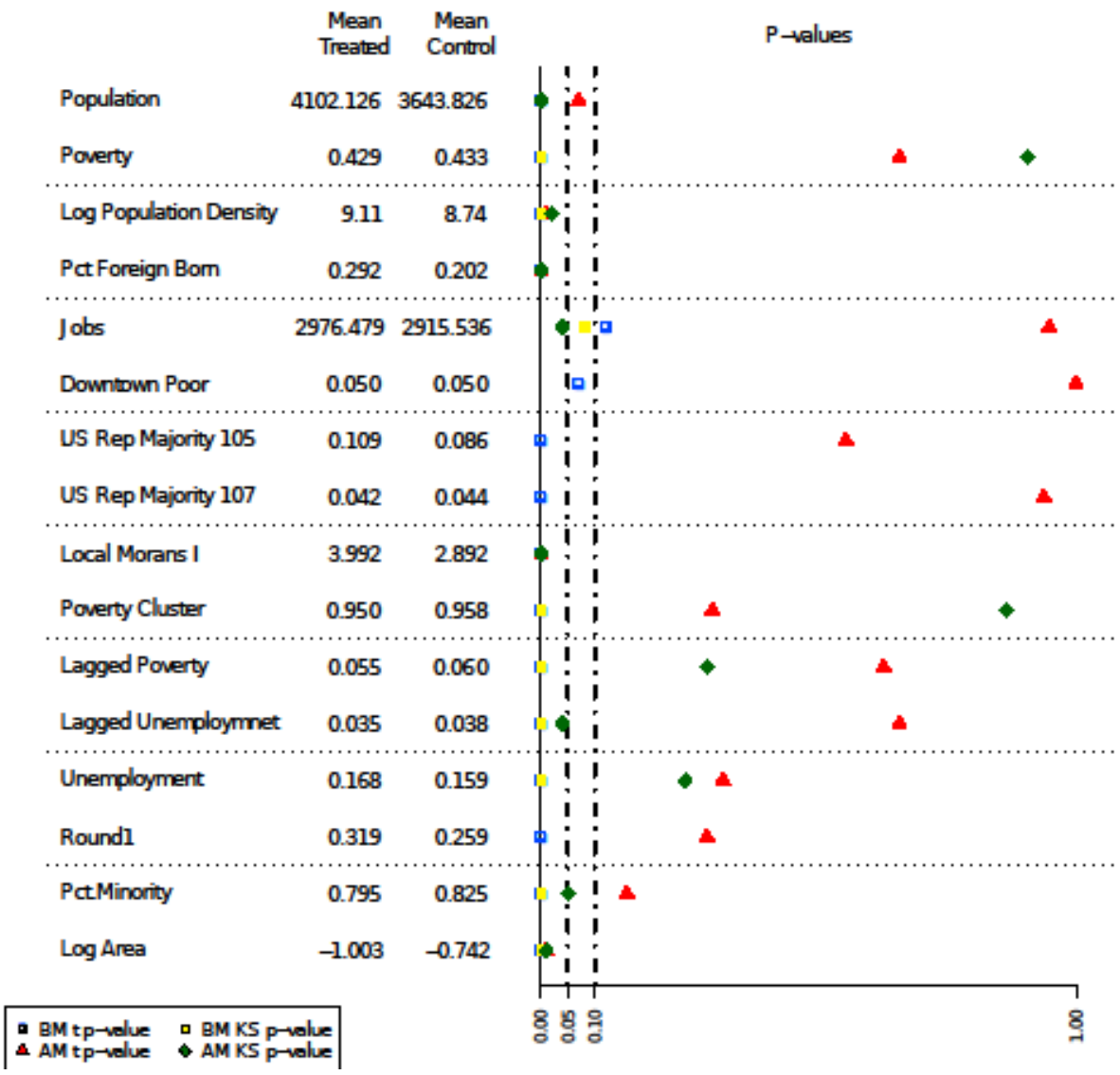

Table 2: Descriptive Statistics of Study Sample from the 1990 Census Neighborhood Change Data Base

	Pop-ulation	Pct. Poverty	Pct. Un-employment	Pct. Foreign Born	Pct. White	Pct. Black	Pct. Asian-Pacific Islander	Pct. Other	Pct. Hispanic
Chattanooga, TN Urban RC	8,863	61.6	63.8	.4	9.4	90.4	.0	.0	.2
Fresno, CA Urban EZ3	47,582	42.1	52.9	22.6	29.9	22.9	14.4	31.8	45.3
Knoxville, TN Urban EZ2	47,866	40.2	43.8	2.2	61.4	36.6	1.4	.2	.4
Los Angeles, CA Urban RC	197,957	36.3	39.6	59.6	21.9	18.3	8.1	51.4	71.3
Memphis, TN Urban RC	145,162	49.2	49.7	.4	5.7	94.0	.2	.1	.3
San Diego, CA Urban RC	96,171	37.4	44.9	38.2	29.2	25.1	5.8	39.2	56.3
San Francisco, CA Urban EC	29,955	33.8	43.2	38.5	28.5	33.6	15.7	21.4	38.2
San Francisco, CA Urban RC	40,000	27.0	42.7	44.1	47.7	9.8	34.8	6.6	14.9
Santa Ana, CA Urban EZ2	49,301	28.8	34.0	57.6	75.3	1.4	5.5	17.4	85.7

Table 3: Impacts of Wage Credits and Grants on Jobs, New Establishments, and Establishment Closures: Pooled Estimates for All Cities.

	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
Variable	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	1.024*	1.047	0.926*	1.029	0.580***	0.941	1.212***	1.092	0.939	1.455	0.853	0.657
EZ Grant Level	0.997	0.660***	0.836**	0.949*	0.764**	0.857	0.938***	0.603***	0.843**	1.147	0.556	1.134
Wage Credit Trend	1.013	0.961*	0.992	1.003	1.134***	1.006	1.035	0.952**	0.966*	0.847*	0.948	0.868
EZ Grant Trend	0.999	1.045***	1.008	0.987	1.001	1.001	1.023	1.065***	1.012	1.012	1.103	1.019
n	6624	6256	6256	6624	6256	6256	6624	6256	6256	6624	6256	6256

*Note 3: * p<0.05; ** p<0.01; *** p<0.001*

Table 4: Change in Rates Per Census Tract in Select Empowerment Zones

	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
FRESNO, CA EZ	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	0.970	1.260***	1.134	1.050	0.748*	1.309	1.191***	1.371***	1.425***	1.085	.	0.042***
Wage Credit Trend	0.996	0.947*	0.977	0.977	0.982	0.982	1.028	0.920**	0.942	0.819*	.	2.213**
n	576	544	544	576	544	544	576	544	544	576	*544*	544

	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
KNOXVILLE, TN EZ	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	.	0.619***	1.366	1.048	0.542***	0.690	1.248**	0.611**	1.898**	1.248**	.	4.081***
EZ Grant Level	.	0.413***	0.877	1.050	0.511***	0.740	0.941	0.305***	0.932	0.941	.	2.021
Wage Credit Trend	.	0.649***	0.948	1.398*	0.739*	0.887	1.065	0.560***	0.947	1.065	.	0.614
EZ Grant Trend	.	1.539***	1.031	0.739*	1.484***	1.060	1.034	1.757***	1.007	1.034	.	1896.3***
n	828	782	782	828	782	782	828	782	782	828	*782*	782

	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
SANTA ANA, CA EZ	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	1.056**	1.039	0.974	1.039***	0.529***	1.081	1.104	1.054	0.991	2.885**	32.912*	4.158
EZ Grant Level	0.993	0.555***	0.780**	0.983	0.586***	0.738	0.949*	0.472***	0.772**	1.004	1.916	2.801
Wage Credit Trend	1.029	0.788***	0.937	1.037	0.916	0.881	1.227*	0.760***	0.920	0.410**	3.579	1.692
EZ Grant Trend	1.000	1.208***	1.040	0.971	1.222**	1.115	0.921	1.225***	1.044	2.074*	0.202*	0.618
n	612	578	578	612	578	578	612	578	578	612	578	578

Note: Convergence not achieved for Fresno minority new firms, Knoxville's all eligible businesses' jobs, and minority new firms. No overall Wald test could be calculated for minority firm closures in Knoxville due to the data structure. * p<0.05; ** p<0.01; *** p<0.001.*

Table 5: Change in Rates Per Census Tract in Select Renewal Communities

	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
CHATTANOOGA, TN RC	Jobs	New	Closures	Jobs	New	Closures	Jobs	New	Closures	Jobs	New	Closures
Wage Credit Level	1.065	0.965	1.129	1.098*	0.438***	0.935	1.044	0.973	1.177	1.044	.	.
Wage Credit Trend	1.075*	1.081	1.014	0.938	1.188	1.014	0.920	1.064	0.939	0.920	.	.
n	180	170	170	180	170	170	180	170	170	180	170	*170*
	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
LOS ANGELES, CA RC	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	1.036	0.833***	0.829*	1.055*	0.582***	0.983	1.184***	0.871*	0.845*	0.934	0.804	0.175***
Wage Credit Trend	0.994	0.998	0.974	0.925**	1.071	0.937	1.031	0.992	0.959	0.976	0.945	1.455*
n	1440	1360	1360	1440	1360	1360	1440	1360	1360	1440	1360	1360
	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
MEMPHIS, TN RC	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	1.030	1.263**	0.894	1.022	0.756	1.001	1.089	1.299**	0.816	1.089	0.476	0.177
Wage Credit Trend	1.037*	1.047	1.071**	1.061***	1.262***	1.057	1.011	1.085*	1.031	1.011	1.986	1.103
n	1728	1632	1632	1728	1632	1632	1728	1632	1632	1728	1632	1632
	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
SAN DIEGO, CA RC	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	1.086**	1.457***	0.813**	1.013	0.738	0.754*	1.444**	1.629***	0.891	1.209	0.157***	0.172*
Wage Credit Trend	0.954**	0.904**	1.025	0.935**	1.114*	1.064	0.918	0.900**	1.067	0.660*	1.220	2.301**
n	828	782	782	828	782	782	828	782	782	828	782	782
	All Eligible Businesses			Retail Businesses			Five Employees or Fewer			Minority Businesses		
SAN FRANCISCO, CA RC	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures	Jobs	Openings	Closures
Wage Credit Level	0.982	1.248***	0.877*	1.012	0.539***	0.672***	*0.011***	1.387***	0.777***	1.052	0.541	0.270***
Wage Credit Trend	0.948***	0.997	0.937	0.930***	1.235***	1.027	*2.390***	0.956	0.907	1.014	2.027	1.036
n	432	408	408	432	408	408	*432*	408	408	432	408	408

Note 5: Convergence not achieved for Chattanooga minority new firms and minority firm closures. No overall Wald test could be calculated for Chattanooga's minority employment and firms with five employees or fewer due to the data structure. * p<0.05; ** p<0.01; *** p<0.001*